gases, &c.; the steam generated represents 896 units, and of these 112 are lost by condensation, &c. The steam supplied to the engine represents 784 units, and of these 667 are lost in the exhaust, so that only 117 are converted into indicated work, and from this 17 are deducted for friction. In Fig. 4 525 heat units are absorbed in the producer, and of these 105 are taken as lost in ashes, radiation, cooling of gas, &c. The gas supplied to the engine represents 420 units, and, as in Fig. 3, 117 units are converted into indicated work, and of these 17 are deducted for friction. In Fig. 5 1680 heat units are absorbed in the boiler, and of these 420 are lost in ashes, radiation, &c.; the steam generated represents 1260 units, and of these 84 are lost by condensation, &c. The steam supplied to the engine represents 1176 units, and of these no less than 1059 are lost in the exhaust. In Fig. 6 494 heat units are absorbed in the producer, and of these 74 are taken as lost in ashes, radiation, cooling of gas, &c. The gas supplied to the engine represents 420 units, and the remaining losses are similar to those in Fig. 4.

On these bases the general result is that for the 250-B.H.P. size, in order to obtain 100 heat units in useful work with steam power there must be 1120 heat units in the fuel consumed in the boiler; whereas with gas power there need only be 525 units in the fuel consumed. This shows a saving of 53 per cent. in the weight of fuel in favour of the gas plant. The result is still more striking in the case of the 40-B.H.P. size, as there must be 1680 units in the fuel consumed for steam power compared with 494 for gas power. This is a saving of 70 per cent. in favour of the gas plant. These figures do not include any allowance for stand-by losses, which would be considerably less for gas than for steam power.

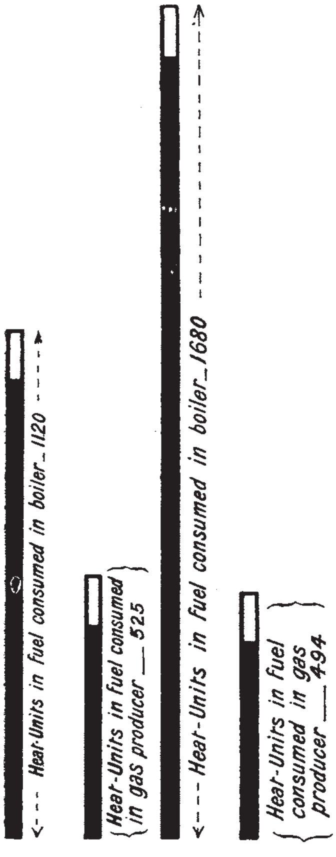

Fig.3. Fig.4. Fig.5. Fig.6.

FIG. 3.—250 B.H.B. steam.
FIG. 4 —250 B.H.P. gas.
FIG. 5. -40 B.H.P. steam.
FIG. 6.—40 B.H.P. gas.

After considering the two types of plant, I think our general conclusions may be as follows:—A suction plant has certain practical advantages—it costs less and occupies a smaller ground-space; but the gas made in it is not so strong as in the older form of pressure plant, and in the case of large engines this advantage may be important, as it affects the maximum power of the engine. The fuel consumption per H.P.-hour and the labour required are about the same in both types of plant, provided the steam required is raised without an independent boiler. The consumption of water is the same in both types. Where there are several engines to serve, a pressure plant is better, as all can be served with one main from the gas-holder, with a branch to each engine. This simplifies the piping and reduces its cost considerably; it also facilitates the starting of the engines. It seems to me that each plant has its own province, and that in some cases the pressure type is better than the suction type; in others suction is better than pressure.

Looking at the matter broadly, one cannot but be struck with the enormous development in gas power which has taken place during the last ten, and especially during the last five, years. Small steam engines are being rapidly superseded, and in several cases the makers of steam engines are now making gas engines. At first only small gas engines were supposed to be within the range of practical politics, but those days are over, and there are many gas engines developing more than 1000 H.P. each which are working satisfactorily. Gas power has come to stay, and now has a recognised position among engineers.

J. EMERSON DOWSON.

TRANSATLANTIC WIRELESS TELEGRAPHY.[1]

ON previous occasions I have had the honour of describing before this institution some of the stages through which the application of electric waves to telegraphy through space has passed. This evening I propose to confine myself chiefly to describing the results and observations recorded during the numerous tests and experiments which my collaborators and I have been carrying out with the object of proving that wireless telegraphy across the Atlantic was possible, not merely as an experimental feat, but as a new and practical means for commercial communication (Journ. Inst. Elec. Eng., xxviii., 1899, p. 291).

In March, 1899, communication was established by means of my system of wireless telegraphy across the Channel between England and France (see Fig. 1), and the *Times* of March 29 of that year published the first Press telegram ever transmitted to England from abroad by means of electric-wave telegraphy.

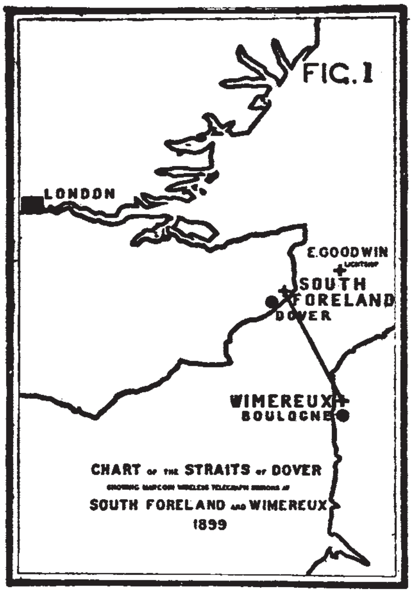
FIG. 1
LONDON
E. GOODWIN LIGHTSHIP
SOUTH FORELAND
DOVER
WIMEREUX
BOULOGNE
CHART OF THE STRAITS OF DOVER
SHOWING MARCONI WIRELESS TELEGRAPH STATIONS AT
SOUTH FORELAND AND WIMEREUX
1899

At that time a considerable discussion took place in the Press as to whether or not wireless telegraphy would be practicable for much longer distances than those then covered, and a general opinion prevailed that the curvature of the earth would be an insurmountable obstacle to long-distance transmissions, in the same way as it was, and is, an obstacle to signalling over considerable distances by means of optical signals such as flashlights, the heliograph, or the semaphore.

Other difficulties were anticipated as to the possibility of being able practically to employ and control a transmitter capable of radiating an amount of electrical energy large enough to actuate a receiver at really great distances, and, granting the possibility of this, whether such a powerful radiator would not interfere with the working of all other wireless stations which might be established on shore or ships within the sphere of influence of the long-distance sender.

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[1] From a discourse delivered at the Royal Institution on Friday, March 13, 1908, by Commendatore G. Marconi.

What so often occurs in most pioneer work has repeated itself in the case of long-distance wireless telegraphy—the anticipated obstacles and difficulties were either imaginary or else easily surmountable; but in their place unexpected barriers manifested themselves, and my efforts and those of my collaborators have been mainly directed to the solution of problems presented by difficulties which were not anticipated when the tests over long distances were first initiated.

FIG. 2

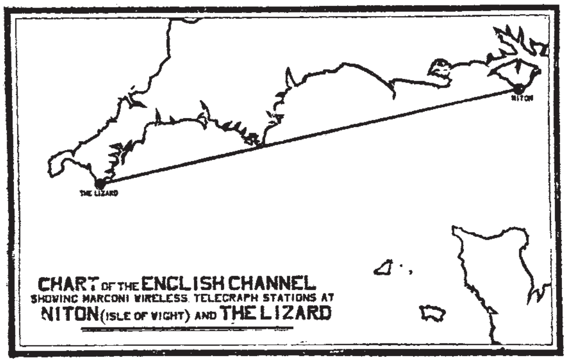


CHART OF THE ENGLISH CHANNEL
SHOWING MARCONI WIRELESS TELEGRAPH STATIONS AT
NITON (ISLE OF WIGHT) AND THE LIZARD
NITON
THE LIZARD

In January, 1901, wireless communication was established between St. Catherine's Point in the Isle of Wight and Lizard in Cornwall, over a distance of 186 miles. The height of these stations above the sea-level did not exceed 300 feet (100 metres), whereas to clear the curvature of the earth a height of more than a mile at each end would have been necessary.

The result of these tests went far to convince me that electric waves produced in the manner I had adopted were able to make their way round the curvature of the earth, and that therefore it was not likely that this factor would constitute a barrier to the transmission of waves over greater distances. At this time I had achieved a considerable measure of success, by means of syntonic or tuning devices, in preventing mutual interference between stations, and Prof. Fleming described, in a letter to the *Times*, dated October 4, 1900, the results obtained, and which he and others had witnessed (Journ. Soc. Arts, xlix., No. 2530, 1901). The principle on which the transmitters and receivers at St. Catherine's Point and the Lizard were worked is shown in Figs. 3 and 4.

FIG. 3

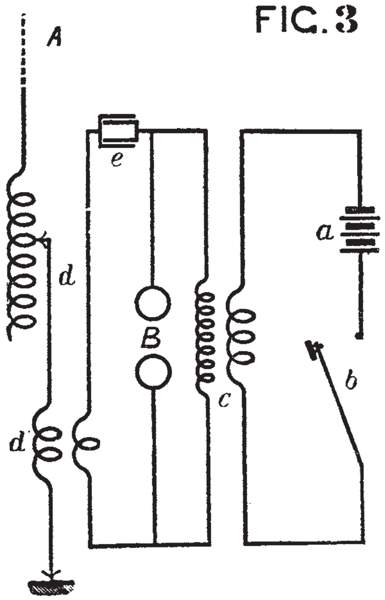
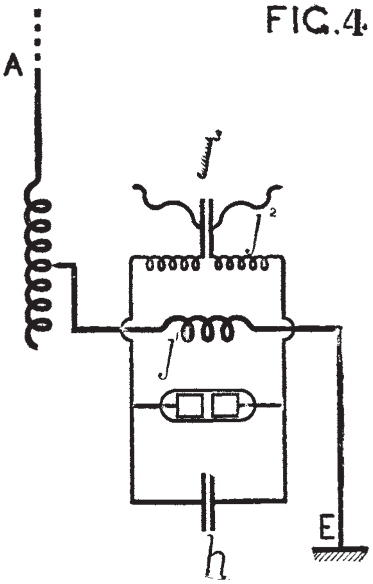


FIG. 4



At the transmitting end a condenser, usually taking the form of a battery of Leyden jars, had one terminal connected to one spark-ball of an induction coil or transformer and the other to the primary circuit of an oscillation transformer. The opposite terminal of this transformer circuit was joined to the second spark-ball. The condenser was charged to the potential necessary to produce a suitable spark by means of an induction coil. The secondary circuit of the oscillation transformer was inserted between the vertical conductor, or aërial wire, and earth, and an adjustable inductance coil included in the circuit.

The circuits, consisting of the oscillating circuit and radiating circuit, were more or less closely "coupled" by varying the distance between the primary and secondary of the oscillation transformer. By the adjustment of the inductance inserted between the elevated conductor and earth, and by the variation of the capacity of the primary circuit of the oscillation transformer, the two circuits of the transmitter could be brought into resonance, a condition which I first found was absolutely necessary in order to obtain efficient radiation.

The receiver consisted also of a vertical conductor or aërial connected to earth through the primary of an oscillation transformer, the secondary of which included a condenser and a coherer, or other suitable detector, it being necessary that the circuit containing the aërial and the circuit containing the detector should be in resonance with each other, and also in tune with the periodicity of the oscillations transmitted from the sending station.

The energy employed to signal over a distance of 186 miles could be brought as low as 150 watts, and even less if a higher or larger aërial had been used.

The facility with which distances of more than 100 miles could be covered prior to 1900, and the success of the methods for preventing mutual interferences (Journ. Soc. Arts, xlix., No. 2530, 1901), led me to advise that two large power stations be constructed, one in Cornwall and the other in North America, in order to test whether it was possible to transmit messages across the Atlantic Ocean.

I have often been asked why I did not first endeavour to establish commercial communication between places situated at a shorter distance. The answer is very simple. The cables which connect England to the Continent, and between most Continental nations, are Government-owned, and these Governments would not, and will not, allow the establishment of any system, wireless or otherwise, which might in any way tamper with the revenue derived from these cables.

As regards Transatlantic communication, however, the conditions were different. There was no law either here, in Canada, or in the United States to impede the working of wireless telegraphy across the Atlantic.

A further potent reason, moreover, an economical reason, prompted me to attempt communication with America. Notwithstanding the cost of high-power stations, I am convinced that it is more profitable to transmit messages at 6*d.* a word to America than at, say, ½*d.* a word across the Channel, and that the economical advantage of wireless over cables and land-lines increases instead of diminishing with the distance.

A site suitable for a long-distance station was chosen at Poldhu, in Cornwall, and here in 1900 work was commenced in earnest—work in which I was ably assisted by

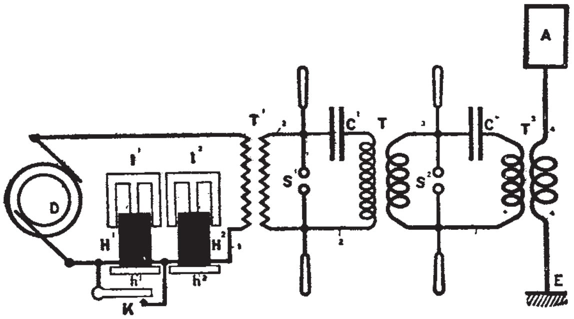
Prof. J. A. Fleming, of the University of London. The transmitter at Poldhu was similar in principle to the one I have already described, but it is obvious that the considerable distance over which it was proposed to transmit signals necessitated the employment of more powerful electromagnetic waves than those ever previously used. These were obtained by means of a generating plant consisting of an alternator capable of an output of about 25 kilowatts, which, through suitable transformers, charged a condenser having a glass dielectric of great strength.

Time does not permit me to describe in detail all the engineering difficulties which were encountered in controlling electrical oscillations of a power which at that time was certainly unprecedented, and as the tests were made possible by commercial organisation, the objects of which do not consist solely in the advancement of science, you will understand that a detailed description of the plant used at the Transatlantic stations cannot, for the present at least, be made public.

My early tests on wireless transmission by means of the elevated capacity method had convinced me that when endeavouring to extend the distance of communication it was of little utility merely to increase the power of the electrical energy applied to the transmitting circuits, but that it was also necessary to increase the area or height of the transmitting and receiving elevated conductors.

As it was economically impracticable to use vertical wires of very great height, the only alternative was to increase their size or capacity, which, in view of the facts I had first noticed in 1895, seemed likely to make possible the efficient utilisation of large amounts of electrical energy (Journ. Inst. Elec. Eng., xxviii., 1899, pp. 278–9).

FIG. 5



The form of aërial which I first proposed to employ consisted of a conical arrangement of wires insulated at the top and gathered together at a lower point in the form of a funnel. This aërial was supported by a ring of twenty masts each 200 feet high, arranged in a circle 200 feet in diameter.

During the first tests an arrangement of circuits (Fig. 5) proposed by Dr. Fleming, and consisting of a modification of the system shown in Fig. 3, was employed. In this arrangement, in place of one high-frequency oscillation circuit, two are employed, and the constants of the two circuits are so arranged that very high-tension discharges can be obtained from one of the condensers—the one which is inductively connected with the aërial—without danger of damage to the circuits of the generator ("The Principles of Electric Wave Telegraphy," 1906, p. 506).

Simultaneously with the construction of the station at Poldhu, the erection of another one on substantially the same plan was undertaken at Cape Cod, in the United States of America.

The completion of the arrangements was delayed owing to a storm, which wrecked the masts and aërial at Poldhu on September 18, 1901, but by the end of November the aërial was sufficiently restored to enable me to complete the preliminary tests which I considered necessary prior to making the first experiment across the Atlantic.

Another accident to the masts at Cape Cod seemed likely to postpone the tests for several months more. I therefore decided that in the meantime I would use a purely temporary receiving installation in Newfoundland for the purpose of testing how far the arrangements in Cornwall had been conducted on right lines.

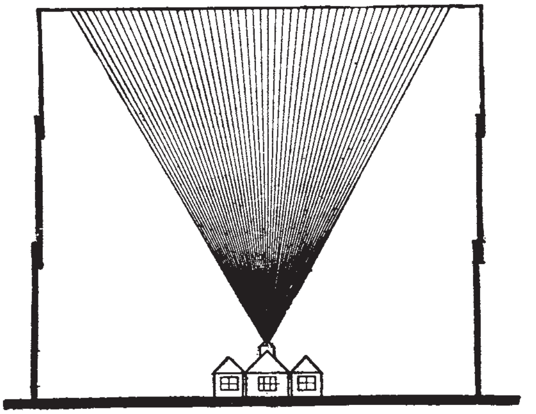
The transmitting elevated conductor employed at Poldhu during the experiments with Newfoundland consisted of fifty almost vertical copper wires supported at the top by a horizontal wire stretched between two masts 48 metres high and 60 metres apart. These wires converged together at the lower end in the shape of a large fan, and were connected to the transmitting instruments situated in a building (Fig. 6).

The transmitting condenser used with this aërial had a capacity of one-fiftieth of a microfarad, and was charged to a potential sufficient to produce a suitable spark discharge between spheres 3 inches in diameter, 1½ inches apart, the wave-length being 1200 feet. The actual power employed for the production of the waves was about 15 kilowatts.

I left for Newfoundland on November 27, 1901, with two assistants. As it was impossible at that time of the year to set up a permanent installation with poles, I decided to carry out the experiments by means of receivers connected to elevated wires supported by balloons or kites—a system which I had previously used when conducting tests across the Bristol Channel for the Post Office in 1897.[1]

It will be understood, however, that when it came to flying a kite on the coast of Newfoundland in the month of December this method was neither an easy nor a comfortable one. When the kites were got up much difficulty was caused by the variations of the wind producing constant changes in the angle and altitude of the wire, thereby causing corresponding variations in its electrical capacity and period of electrical resonance. My assistants at Poldhu, in Cornwall, had received instructions to send on and after December 11, during certain hours every day, a succession of S's followed by a short message, the whole to be transmitted, at a certain pre-arranged speed, every ten minutes, alternating with five minutes' rest.

FIG. 6



Owing to the constant variations in the capacity of the aërial wire in Newfoundland, it was soon discovered that an ordinary syntonic receiver was not suitable, although, at one time, a number of doubtful signals were recorded. I therefore tried various microphonic self-restoring coherers placed either directly in the aërial or included in the secondary circuit of an oscillation transformer, the signals being read on a telephone.

On December 12 the signals transmitted from Cornwall were clearly received, at the pre-arranged times, in many cases a succession of S's being heard distinctly, although probably in consequence of the weakness of the signals and the constant variations in the height of the receiving aërial no actual message could be deciphered. The following day we were able to confirm the result. The signals were actually read by myself and by my assistant, Mr. G. S. Kemp.

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[1] "Signalling through Space without Wires," lecture by Sir William Preece, Royal Institution, June 4, 1897. Proc. R.I., xv., p. 467.

I have often been asked why I adhered to the practice of transmitting series of the letter S for these tests. The reason is that the switching arrangements at the sending station at Poldhu were not constructed at that time in such a manner as to withstand long periods of operation—especially if letters containing dashes were sent—without considerable wear and tear, and that if S's were sent an automatic sender could be employed. Moreover, the immediate object of these experiments was not to transmit actual messages across the ocean, but to ascertain the possibility of detecting the effects of electric waves at a distance of 2000 miles.

The result obtained, although achieved with imperfect apparatus, was sufficient to convince me and my co-workers that by means of permanent stations (that is, stations not dependent on kites or balloons for sustaining the elevated conductor) and by the employment of more power in the transmitters it would be possible to send messages across the Atlantic Ocean with the same facility with which they were being sent over much shorter distances.

About two months later, in February, 1902, further tests were carried out between Poldhu and a receiving station on board the American liner *Philadelphia*, *en route* from Southampton to New York. The sending apparatus at Poldhu was the same as that used for the Newfoundland experiments. The receiving aërial on the ship was fixed to the mainmast, the top of which was 60 metres above sea-level. As the elevated conductor was fixed, and not floating about with a kite, as in the case of the Newfoundland experiments, good results were obtained on a syntonic receiver, and the signals were all recorded on tape by the ordinary Morse recorder. On the *Philadelphia* readable messages were received from Poldhu up to a distance of 1551 miles, S's and other test letters as far as 2099 miles.

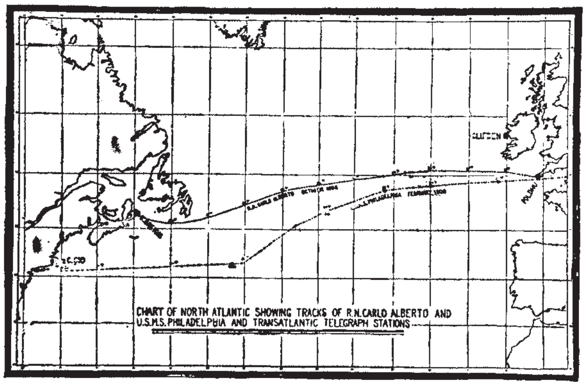
CLIFDEN
POLDHU
CHART OF NORTH ATLANTIC SHOWING TRACKS OF R.N.CARLO ALBERTO AND U.S.M.S. PHILADELPHIA AND TRANSATLANTIC TELEGRAPH STATIONS

Fig. 7.

Although I never had the slightest doubt in my mind as to the genuineness of what was accomplished between Poldhu and Newfoundland, the results obtained on the *Philadelphia* amply prove that the station at Poldhu was capable at that time of transmitting signals to a distance of at least 2000 miles, which is the distance separating Cornwall from Newfoundland, and that if it was practicable to send a message over 2000 miles of sea from shore to ship, it should also be practicable to send it over the same space of ocean from shore to shore.

A result of some scientific interest which I first noticed during the tests on the s.s. *Philadelphia* was the very marked effect of sunlight on the propagation of electric waves over great distances.[1]

At the time of these tests I was of opinion that this effect might have been due to the loss of energy at the transmitter by daytime, caused by the dis-electrification of the highly charged transmitting elevated conductor operated by the influence of sunlight. I am now inclined to believe that the absorption of electric waves during daytime is due to the ionisation of the gaseous molecules of the air effected by ultra-violet light, and as the ultra-violet rays which emanate from the sun are largely absorbed in the upper atmosphere of the earth, it is probable that the portion of the earth's atmosphere which is facing the sun will contain more ions or electrons than that portion which is in darkness, and therefore, as Prof. J. J. Thomson (*Phil. Mag.*, August, 1902, Ser. 6, iv., p. 253) has shown, this illuminated and ionised air will absorb some of the energy of the electric waves.

The fact remains that clear sunlight and blue skies, though transparent to light, act as a kind of fog to powerful Hertzian waves. Hence the weather conditions prevailing in this country are usually suitable for long-distance wireless telegraphy.

Apparently the amplitude of the electrical oscillations and the lengths of waves radiated have much to do with the interesting phenomena, small amplitudes and long waves being subject to the effect of daylight to a less degree than large amplitudes and short waves. I never considered that this daylight effect would be an insuperable obstacle to Transatlantic telegraphy, as sufficient sending energy could be used during daytime to make up for the loss of range of the transmissions.

Turning again to Newfoundland, I ought to add that the experiments could not there be continued or extended in consequence of the hostile attitude of the Anglo-American Telegraph Company, which claimed all rights for telegraphy, whether wireless or otherwise, in Newfoundland.

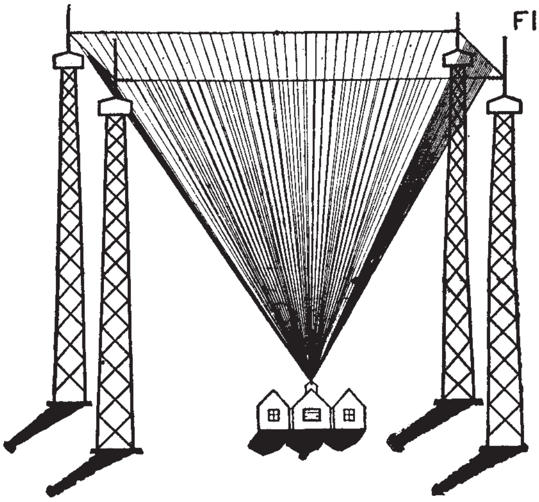

FIG. 8

However, as I had received an offer of assistance from the Canadian Government, it was decided to resume the tests between Great Britain and Canada, and these tests were very greatly facilitated by the subsidy of 16,000*l.* granted by the Canadian Government to support my experiments. The construction of another long-distance station was, therefore, commenced at Glace Bay, in Nova Scotia, and very extensive tests and experiments were carried on with Poldhu during the latter part of 1902.

Contemporaneously with the construction of the station at Glace Bay, alterations and modifications were executed at Poldhu. Four wooden lattice towers, each 210 feet high, were erected at the corners of a square of 200-feet side. The towers carried insulated triatic stays, from which was suspended a conical arrangement of four hundred copper wires forming the aërial, put up in sections, so that more or less could be employed (Fig. 8). The buildings for the generating plant were placed in the middle of the space between the towers. Additional machinery was obtained, and alterations carried out in accordance with the experience obtained from previous tests.

Identical towers and aërial arrangements were at that time adopted at the stations at Glace Bay, and at the similar installation in course of erection at Cape Cod, Mass.

In most of the experiments carried on from Poldhu the capacity of the sending condenser was one-thirtieth of a microfarad, the spark-length 1$\frac{5}{8}$ inches, and the wave-length 3600 feet. In these and subsequent tests the double condenser arrangement of Dr. Fleming was replaced by a single condenser, the arrangement being similar to that shown in Fig. 3.

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[1] Proc. Roy. Soc., lxx., p. 344, "A Note on the Effect of Daylight upon the Propagation of Electro-magnetic Impulses over Long Distances." Paper read June 12, 1902.

During the time that constructional work was in progress at Glace Bay, I carried out some tests with Poldhu over considerable distances, and these tests were greatly facilitated by the interest taken in them by the Italian Government, which placed the cruiser *Carlo Alberto* at my disposal.

During these experiments the interesting fact was observed that, when using waves of more than 1000 metres in length, intervening land or mountains do not bring about any considerable reduction in the distance over which it is possible to communicate. Thus messages and Press despatches were received from Poldhu at the positions marked on the map (Fig. 9), which map is a copy of the one accompanying the official report of the experiments (*Revista Marittima*, Rome, October, 1902).

In December, 1902, messages were for the first time exchanged at night between the stations at Poldhu and Glace Bay, but it was found that communication was exceedingly difficult and untrustworthy from England to Canada, whilst it was good in the opposite direction. The reason for this is that the Glace Bay station was equipped with more powerful and more expensive machinery—a condition rendered possible by the subsidy granted by the Canadian Government; whilst as regards Poldhu, owing to the uncertainty of what might or might not be the attitude of the British Government at that time towards the working of the station, my company was unwilling to expend large sums of money for the purpose of increasing its range of transmission.

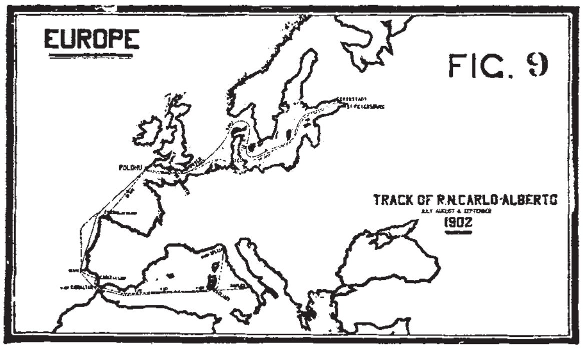

FIG. 9

As, however, messages could be sent then for the first time by wireless telegraphy from Canada to England, inaugural messages were dispatched to the Sovereigns of England and Italy, both of whom had previously given me much assistance and encouragement in my work, and who, by their gracious replies, attested their appreciation of the results which had been achieved. Other messages were also sent to England by the Government of Canada.

Further tests were shortly afterwards carried out with the long-distance station at Cape Cod, in the United States of America, and a message from President Roosevelt was transmitted from that station to His Majesty the King in London.

It is curious to note, in regard to the transmission of this message, that the energy employed at Cape Cod was barely 10 kilowatts, and it was not anticipated that this amount of energy would be sufficient to carry direct to Poldhu. The message was therefore transmitted from Cape Cod, instructions having been given to the operators at Glace Bay to be on the look-out, and to repeat wirelessly on to Poldhu any message received from Cape Cod, and my assistant, Mr. P. J. Woodward, at Poldhu, took in the message on one of my magnetic detectors.[1] The electromagnetic waves conveying this message travelled, therefore, 3000 miles through space over the Atlantic, which distance included about 500 miles of land, following an arc of 45 degrees on a great circle.

(*To be continued.*)

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[1] Proc. Roy. Soc., lxx., p. 341, "Note on a Magnetic Detector of Electric Waves which can be employed as a Receiver for Space Telegraphy."

THE PHYSICS OF GOLF.

IN two articles recently published in the *Times* (March 16 and 23) Sir Ralph Payne-Gallwey has extended in an interesting way the earlier results in the physics of golf which the late Prof. Tait communicated to NATURE between the years 1887 and 1894. In Sir Ralph's experiments the golf balls were projected mechanically by means of a catapult, the ball being either thrown from a cup at the end of the rotating arm or hit off as it hung at the end of a gossamer thread by a blow from the arm. In either case the initial conditions of projection must differ from those which exist in the ordinary mode of propulsion, and it would have been interesting to have had some comparisons. As Tait conclusively showed, the great factor in long driving was the underspin communicated to the ball by the impact of the club in a line below the centre of gravity. Hence the value of the roughened ball, causing not only a better grip between the ball and the club, but also making more efficient the effect of the resistance of the air in producing the uplifting force. Sir Ralph Payne-Gallwey shows experimentally that the ball must not be too much roughened, and that, indeed, a distinctly less roughening than is usual is sufficient to ensure the maximum carry. It is obvious that with a very rough surface the resistance of the air will rapidly cut down the rotation, and thereby diminish the transverse force which lifts the ball against gravity.

Sir Ralph does well in directing attention to the necessity of a truly centred ball. The golfer can readily test the ball in this respect by floating it in water (Tait used to use mercury) and noting whether or not it comes quickly to the same position. If it comes quickly always to the same position it is badly centred, and must be rejected. As all bowlers know, the lack of true centring will give a bias which cannot but produce inaccurate putting. In the flight through the air the bad effects due to the centre of gravity being non-coincident with the centre of figure will probably come into evidence because of the shifting of the axis of rotation. Such a badly centred ball, when projected from the tee, will in general be sent off rotating about an axis which, though initially horizontal, is not a principal axis of inertia. Of necessity precessional motion will result, and the axis of spin will move away from its horizontal position. The phenomena which lead to the evils of slicing and pulling will at once declare themselves. Moreover, if the precessional motion be rapid enough, it is conceivable that the ball might swerve in one direction during one part of its course, but in the other direction during another part of the same trajectory. A sinuous flight is, indeed, occasionally observed, but is generally attributed to the direct action of the wind. Sir Ralph Payne-Gallwey does not himself touch upon the ultimate dynamics of the problem, but confines himself entirely to the direct teaching of experiment. His conclusions are of great practical value to all devotees of the game, and it would be well if the manufacturers of golf-balls would test every ball they put on the market by the simple methods described by him.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Bristol Town Council has decided to contribute in the proportion of one penny in the pound on the rate, or about 7000*l.* per annum, towards the support of the proposed university for Bristol and the West of England, for which more than 200,000*l.* has been subscribed, mainly by members of the Wills family.

THE estimate of the amount required in the year ending March 31, 1910, for grants in aid of the expenses of certain universities and colleges in Great Britain is 217,400*l.* The following are the sub-heads under which this vote will be accounted for by the Treasury. Grants in aid, universities and colleges:—(A) University of London, 8000*l.*; (B) Victoria University of Manchester, 2000*l.*; (C) University of Birmingham, 2000*l.*; (D) University of Wales, 4000*l.*; (E) University of Liverpool, 2000*l.*; (F) Leeds University, 2000*l.*; (G) Sheffield Uni-